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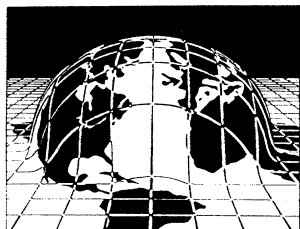
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# PERSPECTIVE

## The Flaring of Intellectual Outliers: An Organizational Interpretation of the Generation of Novelty in the RAND Corporation

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Much of intellectual history is punctuated by the flaring of intellectual outliers, small groups of thinkers who briefly, but decisively, influence the development of ideas, technologies, policies, or worldviews. To understand the flaring of intellectual outliers, we use archival and interview data from the RAND Corporation after the Second World War. We focus on five factors important to the RAND experience: (1) a belief in fundamental research as a source of practical ideas, (2) a culture of optimistic urgency, (3) the solicitation of renegade ambition, (4) the recruitment of intellectual cronies, and (5) the facilitation of the combinatorics of variety. To understand the subsequent decline of intellectual outliers at RAND, we note that success yields a sense of competence, endurance in a competitive world, and the opportunity and inclination to grow. Self-confidence, endurance, and growth produce numerous positive consequences for an organization; but for the most part, they undermine variety. Outliers and the conditions that produce them are not favored by their environments. Engineering solutions to this problem involve extending time and space horizons, providing false information about the likelihoods of positive returns from exploration, buffering exploratory activities from the pressures of efficiency, and protecting exploration from analysis by connecting it to dictates of identities.

**Keywords:** organizational evolution and change; organization and management theory; archival research; organizational processes; organization and management theory; deviance; innovation

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### Introduction

Much of intellectual history is punctuated by the brief flaring of intellectual outliers, small groups of thinkers who briefly, but decisively, influence the development of ideas, technologies, policies, or worldviews. Several of these episodes in the United States were associated with the last 60 years. It is not hard to list examples: Bell Laboratories, the Manhattan Project, the Livermore Laboratories, the Palo Alto Research Center of the Xerox Corporation, the Applied Mathematics Panel of the Office of Scientific Research, the Graduate School of Industrial Administration of the Carnegie Institute of Technology, the Statistical Research Group at Columbia University,

and several others. The organization, funding, locales, and content of these places differ. They include think tanks; university departments; ad hoc wartime groups; and commercial, industrial, and government laboratories.

The flaring of intellectual outliers is a small, but possibly important, aspect of larger stories that are told of innovation in scholarship, technology, and economic and political systems. The search for understanding is plagued by the apparent causal complexity. Scholars looking through the lenses of various disciplines and perspectives (including economics, engineering, industrial and evolutionary dynamics, history, sociology, and history of science) have examined the evolution of

technologies and places (Christensen 1997, Nelson and Nelson 2002, Smits 1985); the coevolutionary dynamics arising from university–industry relations in areas such as chemistry (Murmman 2003) and chemical engineering (Rosenberg 1998); the relations between science, innovation, national and educational systems, and technology and industries (Ben-David 1971, Flexner 1930, Hounshell 2000, Nelson and Nelson 2002, Noble 1977); the relation between properties of organizations and innovativeness (Burgelman 1992, Sørensen and Stuart 2000); trade-offs between flexibility and efficiency in organizations (Adler et al. 1999); the influence within certain organizations or movements of certain (creative/outlier) individuals such as Vannevar Bush (Zachary 2004), Warren Weaver (Weaver 1970), and J. Robert Oppenheimer (Kelly 2006); the difficulties in sustaining creative “hot groups” (Leavitt 1996); issues of industrial policy and the influence of legal frameworks (such as patent law) on innovation (Heller and Eisenberg 1998, Sakakibara and Branstetter 2001); the influence of organizational and institutional structures on creativity in nanotechnology and human genetics (Heinze et al. 2009); how one might try to generate a steady stream of future intellectual and commercial innovations (Jelinek and Schoonhoven 1990); and more general questions concerning the sources of novelty in adaptive processes (March 2010, Chapter 4; Padgett and Powell 2012).

Dramatically favorable novelty is made difficult to understand by the extent to which it appears to be an unlikely outcome of processes that ordinarily yield little reward. Most of the time, it does not happen, even under the most favorable circumstances. One common notion is that favorable novelty is the unanticipated result of unconventional combinations of elementary components (March 2010, Chapter 4). In the ordinary course of events, unconventional mixes do not occur; if they do occur, the results are more likely to be negative or minor than to be dramatically positive. As any child who has played with a chemistry set knows, unconventional mixes are much more likely to fizzle or explode than to turn to precious metal. Numerous unhappy surprises are likely to happen before a happy one occurs, if it ever does. Although some developments are relatively predictable—the processes relating to research and development (R&D), novelty, and innovation—as Charles Hitch observed, the novelty process “resulting in significant scientific or technological advances is universally uncertain, with occasional happy and frequent unhappy surprises” (Hitch 1958, p. 4).

Stories about innovation frequently touch on the role of groups (or individuals within groups) that exhibit the flaring of intellectual outliers, often seeing them as an important source of ideas, but they do not generally try to understand the dynamics of such groups. They are treated as sources of mutations but not as comprehensible parts

of an adaptive system. One exception is Harold Leavitt’s study of hot groups (Leavitt 1996). He discussed two small intellectual groups—one at the Carnegie Institute of Technology around Herbert Simon and his colleagues and the other at the Massachusetts Institute of Technology (MIT) around the Kurt Lewin and Douglas McGregor groups. Despite their differences, Leavitt also finds commonalities between them such as their young age, democratic spirit, high productivity, and (intellectually) competitive nature (Leavitt 1996, pp. 290–291). Case studies that are made of specific instances of flaring suffer generally from the ills of sampling on the dependent variable, trying to understand success by looking only at success. The latter problem is clearly one that we share in this paper.

### The RAND Corporation

One conspicuous example of the flaring of intellectual outliers was the post-World War II flowering of the RAND Corporation. RAND was one result of the post-WWII recognition of the need for a group of scientists working full time on military matters in peacetime. Rather than simply negotiate contracts with university faculty, the Scientific Advisory Board of the Army Air Forces Chief of Staff decided to establish a think tank with elements of being a pure research institution, though initially they believed it had to be housed in an industrial research facility to gain stability and attract top-flight scientists to work for them. Fairly quickly, however, the industrial setting was seen to have major disadvantages, and RAND was constituted as a nonprofit corporation.

Stephen Enke described the creation of the RAND Corporation in 1946 as “something of an accident” (Enke 1967, p. 4), but this accident can be seen as involving a comprehensible evolution from wartime experience with the military uses of science to efforts to extend that experience into peacetime by creating a “think tank” for the United States Air Force. Aspects of the story of RAND are told in several memoirs and histories (Hounshell 1997, Smith 1966), and they are debated in several memos and papers by current and former RAND employees (Bornet 1961, Digby 2001, Enke 1967, Goldhamer 1972, Williams 1961). The stories differ in important respects. John Williams, who wrote several memos and notes about the nature of RAND, observed, “Individuals often perceive clearly what RAND is, what has given it its character, and what made it viable in the first place; but when you combine their images, the picture gets fuzzy” (Williams 1961, p. 1). Despite the differences, the reports all concur in a description of RAND as a place to which brilliant minds were attracted, as a site of unusual intellectual ferment, as a source of ideas that proved to be important, and as an institution that experienced a brief flaring of intellectual outliers followed by a period of more conventionality.

From the beginning, RAND was protected from routine requests from the military establishment, and researchers were encouraged to “think outside the box.” As a result, RAND’s mandate expanded beyond weapons planning for the Air Force to research on decision making and behavior under conditions of uncertainty. As RAND hired more social scientists, its scholars did research across a broad range of social sciences, making important contributions to—and in many cases, inventing—techniques and perspectives in areas such as systems analysis, game theory, linear programming, evolutionary biology, and studies of organizations. They established the intellectual foundations for research on decision making in economic organizations, including applications of game theory to economics, organizational economics, and evolutionary economics (Augier and March 2011). RAND’s contributions to the field included both basic research and applications (Digby 2001).

For about 15–20 years after the Second World War, RAND was a monastery for engineers, physicists, mathematicians, economists, statisticians, and others interested in solving complex decision problems through systematic application of mathematical tools, including statistical decision theory and game theory. RAND operated under the premise that military problems did not conform to disciplinary boundaries and did not often fit a particular academic category very neatly. Frequently, once employees began research projects, the projects would migrate through several departments, involving researchers of different skills. With a small base of deviant thinkers drawn from multiple disciplines, RAND became a major source of ideas, particularly about systems analysis and economics; some would prove applicable for issues in management, defense, and organizations ranging from the Pentagon to the University of California (Enke 1967, Enthoven and Rowen 1961, Hitch 1996, Rowen 1970, Tucker 1966).

We wish to use the RAND Corporation of the 1940s and 1950s—and particularly the mathematics and economics departments—as a basis for speculating about the flaring of intellectual outliers more generally. The focus on RAND is dictated by the practicalities of time and space and is not meant to suggest that its contributions were more noteworthy than those of others or to ignore the idiosyncratic features of the RAND experience. For example, RAND was broader in disciplinary scope than several of the other examples that are often used in the discussions of the postwar enthusiasm for science and the links between national security and physics in places such as the Radiation Laboratory and the Manhattan Project (e.g., Dennis 1994, Forman 1987). RAND history could also be contrasted with those of innovative developments in molecular biology and the role of the Rockefeller Foundation in this development (Kay 1993, 1997).

Four striking features of groups such as RAND are (a) the unpredictability of their flowering, (b) their complicated relations with their sponsors, (c) the generality of their impact, and (d) the short duration of their distinctiveness. They arise in surprising places at unanticipated times, they struggle with a persistent disconnect between their own trajectories and the desires of their masters, they transform thinking over a community much broader than themselves, and they rather quickly either disappear or decline into respectability indistinguishable from numerous other groups and institutions.

We can speculate about the dynamics of such episodes, why successful clusters of intellectual creativity have appeared, and why they have failed to endure. What produces an institution or a cluster of thinkers that becomes an intellectual outlier?<sup>1</sup> What are the mechanisms of collective novelty? Why are the periods of collective intellectual creativity frequently brief? What are the mechanisms of decline? What is the role of individuals, institutions, organizations, ideas, and interests in the dynamics of change? And what other elements (chance, luck, randomness, etc.) influence the developments?

### Research Sources

The present research draws from published reports on (and by) RAND, including annual reports, Bruce Smith’s early institutional history (Smith 1966), RAND’s 25th anniversary volume (RAND 1973), James Digby’s recollection of RAND personalities and alumni magazine pieces about the early days (Digby 2001), and more specialized discussions covering aspects of RAND’s research on topics such as R&D (Hounshell 2000) and game theory (Mirowski 2002).

We have also drawn on a set of archival sources, some published (often in working paper formats), some not, including several authors’ recollections from decades of experiences at RAND (Enke 1967, Goldhamer 1972). Archival sources include the RAND Corporation Archives; Harold Dwight Lasswell papers (University of Chicago); Hans Speier papers (University at Albany, State University of New York); Herbert Simon papers (Carnegie Mellon University); Smithsonian’s National Air and Space Museum Archives; W. Allen Wallis papers (University of Rochester); and personal papers from individuals including Joan Goldhamer, Andrew Marshall, Malcolm Palmatier, Henry Rowen, James Schlesinger, and Gus Shubert.

Finally, we have drawn from interviews done by ourselves and others relevant to understanding the specifics of RAND; these include Joan Goldhamer’s and Digby’s collection of interviews relating to the rise of strategic thinking at RAND (located in Goldhamer’s basement), which includes interviews with Ed Barlow, Nathan Leites, Henry Rowen, James Schlesinger, and Albert Wohlstetter. We also examined a collection of interviews on RAND’s history archived in the National Air and



Space Museum in Washington, DC. Included in this RAND History Project Interviews collection are interviews with Bruno Augenstein, Frank Collbohm, Lloyd Shapley, Gus Shubert, Albert Wohlstetter, and others. We also had our own extensive conversations with individuals such as Armen Alchian, Alain Enthoven, Joan Goldhamer, Malcolm Palmatier, Henry Rowen, James Schlesinger, Gus Shubert, and Sidney Winter.

In general, these materials are richer in analyses and conclusions than they are in unambiguous empirical data, but collectively, they provide a fairly clear picture of RAND as it was experienced by people who were there in the 1950s and early 1960s. To capture some aspects of the RAND experience as it is reflected in these materials, we quote often from the archival records.

### Untangling the Dynamics of Flaring

Understanding the flaring of intellectual outliers may be important, but it confronts three conspicuous complications. The first is sampling on the dependent variable. It is essential to examine instances of intellectual flaring in order to understand them, but it is hard to understand episodes of flaring simply by looking at those episodes. Whether the prior and contemporaneous factors associated with flaring are its causal determinants cannot easily be assessed by observing exclusively events where flaring is realized. However, it is by no means obvious what an appropriate comparison group would be in the absence of some tentative ideas about the mechanisms involved.

The second complication is identifying causal mechanisms in a probabilistic world. Flaring is a property of many simple stochastic models of combinations. Independent random samples will occasionally produce extreme values that do not persist in subsequent trials. If enough groups are formed (randomly) and produce outcomes that are (random) draws from a distribution of possibilities, a few will be outliers. But that status is not indicative of any special generating properties, nor is it likely to persist through subsequent combinations of individuals and draws of outcomes. Thus, random mechanisms can produce episodes of brief outliers that observers may well attribute to more determinate causes. The classic case with respect to individual biography is the tendency to attribute outliers to the effects of comprehensible life histories or intentional choices rather than to the capability of random reproductive combinations of genes and random social combinations to generate outliers.

The third complication is distinguishing the telling of history from its unfolding. Much of our knowledge about the flaring of outliers comes from interviews with and memoirs written by individuals who were present. With these data, there is a problem distinguishing a shared picture of reality from reality itself. The broad story

we tell is a story that is shared among almost all of the participants. There are differences, but there is also a broad consensus, a consensus that is shared between early accounts and later recollections. As is well known, however, a widely shared view can deviate substantially from reality.

That similar novae are also conspicuous in stories of science, art history, and literary history might suggest that flaring is conspicuous as much because of its appeal to the storytellers of history as because of its validity as history. Individual and organizational memories are imperfect, incomplete, and sometimes distorted. The telling of intellectual history achieves appeal among academic audiences by glorifying human scholars, by emphasizing dramatic events, and by confirming academic prejudices about proper organizational arrangements. Perhaps what we have identified is something important—the predilection of human historians for anointing clusters of human heroes as revolutionary cadres of progress, for confirming prior prejudices, and for inventing comprehensible fables to explain random events—but nothing much about underlying historical reality.

We are aware of, and suitably humbled by, those complications. Our ambitions are therefore appropriately modest. We wish to use some observations of the flaring of intellectual outliers at RAND to generate a few little ideas about the mechanisms involved. They are ideas that we believe are consistent with the observations, but they clearly are not demonstrated conclusively by what we and others have observed. They are speculations about possible histories (Tetlock 1999). And as other histories of ideas and sciences, they are neither linear nor random. Although often evolving in phases of paradigms, ideas have their own centrifugal and centripetal forces. And although there are differences between Kuhn (1970) and Merton (1938) on paradigm development and change, both emphasize the need for “novelty” in ways consistent with our discussion.

### Speculating About the Seeds of Success at RAND

Almost all histories of RAND, as well as histories of other institutions credited with producing intellectual outliers, emphasize the importance of key individuals. At RAND that included individuals such as Armen Alchian, Kenneth Arrow, Bernard Brodie, Daniel Ellsberg, Stephen Enke, Alain Enthoven, Abraham Girschick, Herbert Goldhamer, Jack Hirschleifer, Charles Hitch, Herman Kahn, Burton Klein, Harold Lasswell, Nathan Leites, Harry Markowitz, Margaret Mead, Oskar Morgenstern, Richard Nelson, Roy Radner, Henry Rowen, Thomas Schelling, James R. Schlesinger, Lloyd Shapley, Martin Shubik, Herbert Simon, Robert Solow, Hans Speier, John von Neumann, John Williams, Sidney Winter, and Albert Wohlstetter. They are credited with

major contributions not only to the intellectual outliers that were generated but also to the character and culture of the institution. Shapley (who was awarded the Nobel Prize in Economics for work done largely at RAND) recalled about Williams, “The personalities shape things a lot. Williams... was, from my point of view, the ideal kind of department head. Under him we never had to write a progress report.... Any other bureaucrat says, every six months you have a progress report, what have you done, all of that. He considered it was his duty, as head of the department, to know what his people were doing.”<sup>2</sup> Wohlsetter noted how personalities drove the intellectual content, which became embedded in the larger interdisciplinary collegial environment.<sup>3</sup> Allen Wallis wrote to Frank Collbohm regarding the (very) early organization of RAND divisions, arguing for less planning and more allowance for organic growth: “The organization of research is something I am always reluctant to plan, since it seems to me important that it grow out of the work and the personalities.”<sup>4</sup>

Several recollections about Collbohm’s leadership, personality, and management style credit his uncompromising insistence on the independence of RAND’s research activities (from the Air Force) and his ability to manage by raising questions that would then lead to interdisciplinary discussions and (sometimes) collaboration. Ed Barlow recalled, “[Collbohm] felt very strongly that we must have intellectual independence from our client—the Air Force. They should not tell us what to study or even when to have a particular result and certainly not what the answers should be. He felt that this independence was a core ingredient of what made us a really worthwhile advisor and somewhat different from other ‘think tanks’” (quoted in Digby 2001, p. 11). Gus Shubert also recalled Collbohm’s down-to-earth style, which may have contributed to the culture of respecting ideas, not titles: “RAND was probably the only place in the world where the President was called ‘Frank’ and the coffee man known as ‘Mr. Wilson’... [Frank’s] objective was to have RAND known by its research and by the individuals on the research staff. Consequently, Frank kept his public profile low, almost always deferring, as we would put it, to the ‘important people around here, those who do the research’” (quoted in Digby 2001, p. 18).

Although it would be foolhardy to deny the role of personalities in innovative thinking, broader factors also played a role. In particular, we will focus on five themes that are clearly echoed in the findings from studies of research centers in nanotechnology and human genetics in Europe and the United States carried out by Thomas Heinze and his colleagues (Heinze and Bauer 2007; Heinze et al. 2007, 2009). The themes are a mixture of contextual factors and individual and organizational factors, a mixture of things that were to some extent choices and things that did not exhibit strong elements

of agency. For the most part, it is not possible to assign relative weights to these factors. It is tempting to assert that each was necessary and none was sufficient for the flaring, but there is no way to use the present data to make any such statement, or any other beyond observing that each of the themes seems important in this case.

The first of the themes is a belief in fundamental research as a source of practical ideas. The second is a culture of optimistic urgency. The third is the solicitation of renegade ambition. The fourth is the recruitment of intellectual cronies. The fifth is the facilitation of the combinatorics of variety. Each contributed to producing outliers but did not ensure them. And all five elements contributed to the community-building aspect that was so central to the early RAND successes.

### Belief in Fundamental Research as a Source of Practical Ideas

From the start, the RAND Corporation faced a classic dilemma for scholarship—the relation between “relevance” and “autonomy.” After the Second World War, the RAND Corporation was justified by its expected future contribution to improving strategic and tactical thinking in the United States and particularly in the U.S. Air Force. Enthoven and others point to the fact that the focus affected young minds and inspired altruism in scholars who could have gone off to publish in *Econometrica* but felt that they wanted to “help the country.”

RAND was an arena in which the pursuit of relevance conflicted with the perceived virtues of autonomy in scholarship. There was a respect for the mission of the institution, but much of the work was more traditional and published in academic journals (e.g., Hitch 1955, Kahn and Marshall 1953). In the catechism of academe, scholars are expected to pursue knowledge without regard for external demands. Ideas are to be generated and developed by independent minds pursuing concepts and speculations without regard to the wishes, expectations, and sentiments of others. Knowledge is to be sought for its own sake, or at least without explicit consciousness of its utility. RAND was seen as dealing with issues of national security, but those were very broad indeed. As Rowen noted in his presentation to the RAND board in 1968,

RAND has been widely thought of as being concerned with problems of national security. This has indeed been a principal focus of Rand’s work, but security has been defined in an appropriately broad way. More than fifteen years ago at RAND, [Paul] Samuelson and [Tjalling] Koopmans were working on basic problems of economics, [Allen] Newell and [Oskar] Morgenstern on organization theory. [Paul] Lazarsfeld on measures of attitude, [W. V.] Quine on social welfare, and Goldhamer and Marshall on long-term trends in mental health. RAND has also—almost unnoticed—produced from its very start in the late 1940s some fundamental contributions on the theory of democratic choice. We sponsored Kenneth Arrow’s

initial and best-known work on social choice in a democracy, Herbert Simon's well-known papers on limited rationality in decision making, some of C. E. Lindblom's early work on market-like mechanisms of bargaining in pluralist governments, Anthony Down's writing on bureaucracy, Lloyd Shapley's research on the mathematics of the solution of conflicting choices among many parties, and others. (Rowen 1968, p. 4)

Several of the early RAND leaders were vehement in their support of research autonomy. Shubert recalled his early perception of the RAND mission: "My perception was that it was to be the innovator . . . the hair shirt, to the Air Force . . . RAND was billed to me as an institution which has excellent people in it who know what they're doing, who are free to say what they think on the basis of the work that they've done, whether or not anybody likes it. That was really just what I was looking for, so it was with enthusiasm that I joined, and the enthusiasm for that mission has never flagged in my mind."<sup>5</sup>

Wohlstetter described himself as attracted to RAND because of the ability to engage in basic research and the "enormous latitude" at RAND early on. He was pleasantly surprised to find RAND publishing on issues (such as geometry), which "didn't seem to me to have much directly to do with strategic bombing or anything of that sort."<sup>6</sup> Another early RAND scientist, Olaf Helmer, noted in an interview that "RAND differed very much from other organizations that were given specific assignments. There was an atmosphere at RAND which was even freer in some respects than what you would find at the universities."<sup>7</sup> Williams also noted that "[w]hile RAND might exist without some of the things it enjoys, without independence it doesn't matter whether it exists" (Williams 1961, p. 7), emphasizing that lack of independence would compromise both objectivity and integrity.

The tension between the relevance of scholarship for social problems and the autonomy of scholarship fills the literature and institutions of scholarship without any significant progress toward resolution over time (Augier and March 2011, Chapter 10). A characteristic feature of institutions and cultures that foster intellectual outliers is an interpretation of history that credits fundamental research with important contributions to solving practical problems, the adoption of an ideology that denies the conflict and proclaims—in Abraham Flexner's terms, the "usefulness of useless knowledge" (Flexner 1939). Flexner's call for basic research was in the context of medical education and medical schools (Flexner 1910), and Williams (1961) explicitly linked the importance of fundamental research at RAND to a comparison with the medical profession. A similar call was heard in the context of business schools and business education (Augier and March 2011, Chapter 5).

Confidence in the veracity and centrality of the Flexner epigram was characteristic of RAND, as it was of other institutions that have exhibited intellectual outliers

(Gehani 2003, Hoddeson 1981). Reports of instances of the usefulness of useless knowledge abound and were frequently cited. Enthusiasts proclaimed that the work at RAND illuminated quite important issues in the strategic competition during the Cold War, issues ranging from location of strategic air bases to economic effects of thermonuclear war and beyond.<sup>8</sup> To our knowledge, no serious effort has ever been undertaken to estimate the presumably small likelihood of useless knowledge being useful or to render an unambiguous assessment of the costs and benefits involved. The Flexner epigram about the usefulness of useless knowledge has been accepted (or rejected) more as an article of faith than as a proven (or provable) proposition. This was as true at RAND as it was at other institutions.

During the 1940s and 1950s, there was never any doubt that RAND worked for the Air Force. However, as Enthoven and others noted in personal conversations, a large part of the early success was attributed to the inclination to focus on finding out what needed to be studied—the questions, rather than the answers (RAND 1973, p. ix). There is a problem, however, in attributing research autonomy at RAND exclusively to the enthusiasm of individuals inhabiting RAND. The sense of research autonomy at RAND could be attributed as much to the culture and attitudes of the Air Force as to the culture and attitudes at RAND. After the Second World War, significant senior Air Force officers were as convinced of the value of research autonomy as were RAND researchers. Over time, the officer corps in the Air Force drifted away from a strong support for Flexner and his heirs, with clear effects on the ability of RAND to maintain a pure position in favor of research autonomy.

### A Culture of Optimistic Urgency

The postwar culture of danger and hope extended well beyond RAND, but it was particularly notable there. RAND was created at a time in which scholars were generally optimistic about science (including social science) and about solving urgent world problems through research and analysis. There was a real sense of urgency—especially in the late 1940s to the early 1960s, a sense that a major war with the Soviets was a distinct possibility.

Postwar concerns about sustaining American international preeminence and winning the Cold War (while avoiding a hot one) activated many academic minds. Enthoven, for instance, recalled how he, fresh from the MIT economics Ph.D. program, was attracted to RAND because of the problems. Williams also felt that the dangers of the world warranted his (and others') participation. He said that as a result of the war he had become "very much alarmed at the mass of characters that were loose in the world and decided that there was no one standing between me and these people except the United States armed forces. Then I decided that if I didn't



somehow participate I'd have only myself to blame if I didn't like the way it turned out" (Bornet 1962, p. 4).

There also was a sense of opportunity: there were big new problems (such as intercontinental nuclear war) inviting scientific solutions that were imaginable. Leites, for instance, recalled,

There was a sense of the social sciences being in a pre-Newtonian phase—comparable to the late medieval period in physics—but that a breakthrough might be immanent and might occur at RAND by a combination of the following factors: (a) the mingling in research on the great political problems of social scientists and physical scientists; (b) the focusing on the yet unstudied subject matter of nuclear weapons; (c) the access to highly secret data; and (d) the possibility of effectively approaching the top level of government.<sup>9</sup>

This optimism and urgency was expressed, in part, by faith in the “two-way street” between working on real problems and developing analytical frameworks (Lindblom 1997, Simon 1986, Wallis 1980). It inspired “altruism” in scholars who might have spent their life in normal academia but felt compelled to try to help develop an understanding of big national societal problems, rather than (or in addition to) contributing to their disciplines (Weaver 1970). Scholars could work on problems of great significance to national security and hence to public welfare (Smith 1966). In a letter to Oppenheimer regarding how to organize research at the Institute for Advanced Studies, von Neumann noted, “A certain contact with the strivings and problems of the world that surrounds us is desirable and even necessary” (quoted in Rédei 2005, p. 192). In a letter to Commodore Lewis Strauss (U.S. Atomic Energy Commission) in 1946, von Neumann specifically mentioned wartime experiences as affecting his own increasing interest in problems outside his home discipline: “The war introduced me to great parts of mathematical physics and applied mathematics which I had neglected before” (quoted in Rédei 2005, p. 240).

The great puzzle about the urgency that enveloped RAND was the way it was twisted from a short-time horizon effort, applying clearly relevant knowledge to immediate problems to a long-time horizon to discover new directions. There is adequate research to support the idea that urgency normally shortens time horizons rather than lengthens them; but somehow, in this case, to some extent at least, long-time horizons became fashionable. Wohlsetter noted the lack of immediate deadlines: “One of the things that I found attractive at RAND was that you didn't have an urgent deadline. It was not like a government problem, where you have to get something out in two weeks.”<sup>10</sup> In some mysterious way, an urgent pressure for relevance became an urgent pressure for fundamental research and ideas that were relevant in the long term. It is as though a string quartet stranded in a winter snowstorm decided urgently to compose a new fugue rather than start shoveling.

### Solicitation of Renegade Ambition

RAND was not initially a leading intellectual center that would automatically attract mainstream stars of academic circles. In retrospect, RAND became home to outstanding academic scientists, including Nobel Prize winners, but they generally came to the corporation before they were stars. Among the scholars who were at RAND and later received the Nobel Prize were Kenneth Arrow, James Buchanan, Ronald Coase, John Harsanyi, John Nash, Lloyd Shapley, Herbert Simon, Vernon Smith, Robert Solow, and Oliver Williamson. Some of these stars gravitated later, as stars normally do, to institutional centers of conventional academic excellence, but they came early to RAND and have generally acknowledged the substantial influence of RAND on their career and ideas.

Arrow, for instance, explicitly described how he, looking for a research topic for a dissertation, encountered Helmer at RAND:

I went to RAND, and luck was with me. There was a philosopher there...Olaf Helmer, whom I had actually met earlier.... They hired him through a very complicated chain of circumstances, involving Bertrand Russell.... Helmer translated [Alfred] Tarski's elementary textbook on logic and I proof-read it.... When we were at RAND together, Helmer remarked that there was something that bothered him about game theory or about its applications. We wanted to talk about the US, the USSR, and Western Europe as players, but they are not like people, in what sense do they have utility functions? How can we apply game theory where it is essential to have utility functions? Since when does the US have a utility function? “Oh,” I said, “that's nothing. Abram Bergson has written on this type of thing.” “Oh,” he said, “would you write an exposition of this?” Well that was the thing that led to the social choice book.

(quoted in Feiwel 1987, p. 193)

RAND tried to attract scholars of competence equal to that of academic stars but perhaps with generally less gaudy credentials, a greater penchant for intellectual risk taking, and a modicum of intellectual chutzpah that encouraged working outside the bastions of one's own disciplinary credentials. Outstanding minds were attracted to the combination of interesting ideas and interesting problems. They often worked in teams across disciplinary specializations. Charles Lindblom recalled how he was “enormously stimulated by and pleased” to see much higher interaction among researchers than on any university faculty he had ever seen.<sup>11</sup> He also noted that there was an openness and curiosity and desire to learn: “At RAND the ease of intellectual interchange and the frequency of it seemed to be very, very high, compared with Minnesota, and Yale. I thought that was very heady stuff, and these people were, I thought, thinking sharply, pertinently. Although they loved to argue, they weren't hung up trying to win their points. They



were learning from each other and they liked to find a new idea rather than to fight it off as a challenge to their correctness.”<sup>12</sup> This was from Lindblom’s early period at RAND in the early 1950s. A few years later, he wrote an analysis for Hitch on the declining trends at RAND (anticipating what others such as Marshall and Schlesinger would later see as major barriers to RAND as a place for strategic thinking).

High intellectual capabilities and the ambition to be first combined to stimulate an attraction to high-variance options, particularly among those who were not among the academic “chosen.” It did not, of course, guarantee that any particular outlier direction would succeed, but it produced clusters of ambitious intellectual risk takers who gained further stimulus by finding themselves among kindred spirits.

Any propensity for intellectual exploration was continually threatened by its poor prospects of early success and by the inclination of institutions to punish failure. The administrative system depended on exceptional administrative finesse. Robert Specht noted, “Flexibility and competitiveness and some apparently looseness in organization in general are important assets in promoting an imaginative search for new ideas and new relations . . . . The search for a proper balance between too much order and organization and too little—this search is one that is unending and to which we are not to expect a simple and definitive solution” (Specht 1958, p. 2).

A former vice president of RAND, Joseph Goldstein, described the complications of administering RAND projects:

How do these RAND projects start? A one-man project often starts with one man who has an idea . . . it is possible for someone to pick up the ball and run with it—so long as it is an inexpensive ball that he is running with. We can and must afford small projects of the one-man-off-in-a-corner type with little or no administrative review in their early stages. Many of these die aborning; others become productive projects and may either remain one-man ventures or may expand into larger and more expensive undertakings. In the latter case, the problem of allocating our limited supply of manpower becomes a more difficult one, and management at some level becomes involved. The administrator has not only the difficult task of administering research, but also the equally difficult and important job of refraining from administering.

(Goldstein 1961, p. 4)

In its early years, RAND became a home for intellectual refugees, imaginative people whose ideas may have not found a comfortable home in the establishment. RAND often paid slightly better salaries than did academic departments; but at least according to self-reports, the people who came to RAND were attracted more to the freedom and intellectual excitement than the wage. Shapley recalled, when asked about his unusual hiring

early at RAND, “RAND had anomalies. RAND was itself anomalous at that time.”<sup>13</sup> He further recalled that his first impressions were the openness and the intellectually interesting problems: “I’m not all that disciplined in getting places on time or going to bed when I should so that I can get up when I should. And RAND is sort of nice—it’s open all the time, twenty four hours. People work at night, late, and you can just go in there. The other part was that there’s enough interesting here to do. At that time [in 1948], the mission of RAND was very open . . . . The military felt that . . . we should keep in touch with the scientists after the war.”<sup>14</sup> A similar spirit has been noted in other sites for intellectual outliers. Hiltzik (2000) noted, for instance, that researchers at the Palo Alto Research Center of the Xerox Corporation were attracted by “the thrill of pioneering,” and one of them compared it “to the sheer joy of making the very first footprints in a field of virgin snow” (p. xx).

Shapley argued that it was the relative intellectual freedom that (1) enabled the collection of outstanding outlier minds and (2) allowed them to pursue their ideas with minimal direction. He described the spirit as being “give them some money and say, ‘you think of some problems and tell us about it.’ . . . This led to people like Williams putting together a rather motley crew of people. He hired philosophers . . . And he hired crazy students from the math departments—me.”<sup>15</sup> Marshall recalls that Shapley would work during the nights and sleep during the days without censure from RAND executives. Barlow, an early engineer, also recalled how intellectual freedom became a major factor in attracting good minds to RAND: “One unusual feature is that RAND insisted right from the start on complete intellectual freedom.”<sup>16</sup> It is, of course, somewhat misleading to say that “RAND insisted” when the primary dynamics came from the recruitment of an interlocking group of intellectual activists who believed in the essential necessity of intellectual freedom for scientific development. The recruitment encouraged a culture that was then solidified by the mutual support of the principal actors.

Some of the ideas they worked on early on were seen as “wild” then but are now almost taken for granted. As Augenstein, an early RAND aeronautical engineer, noted,

I think I myself and a lot of other people were attracted to RAND because it had a reputation, even in its infancy, of being considered wild or outrageous or too far removed in time or at great many other considerations, where ideas like that would not only be entertained by they could be developed. And I think that was really kind of the trademark at RAND, in the first decade or two that it was in existence. Many ideas which we take for granted today were quite outrageous 30 or 40 years ago.<sup>17</sup>

Managers such as Williams and Hitch protected the mavericks. Enthoven (1995) mentioned how Hitch, then head of the RAND Economics Department, encouraged

people to think outside the box and stick their necks out. As the leader of the group, he was prepared to take any heat that might come from deviant ideas so that the scholars were protected and could continue their work. Although one cannot attribute RAND's (or any other organization's) success to a few individuals alone, individuals *do* matter, and Hitch is an example.<sup>18</sup> Both in his writings and in his management of the economics department at RAND, Hitch was an organization man. His understanding of the key role of organizations was present in his early critique of marginalist pricing (Hall and Hitch 1939). But even more impressive was his awareness of the dynamics of organization, evident in his building the economics department at RAND, the systems analysis group at the Pentagon, and the University of California. His manners and modesty, as well as his ability to collect people socially (often gathering groups together for dinner and wine at his house), contributed to good people being attracted. And once there, they trusted his leadership. He also had a reputation for uncompromising academic and intellectual integrity. For example, he resisted having senior researchers affix their names to reports prepared by their juniors.

The result was an unusual openness to novel ideas. For example, game theory was welcomed and housed in RAND before it was respected academically (Mirowski 2002). As early as 1946, Ed Paxon, a RAND engineer, discussed with von Neumann the application of von Neumann's and Morgenstern's ideas to naval tactics, and Paxon found it potentially illuminating for different types of conflict.<sup>19</sup> Evolutionary economics, too, had a significant presence at RAND in the 1950s. Some other ideas that were nurtured ultimately became successful as academic subjects, sometimes with considerable delay. These included ideas pursued at RAND in the 1950s about vacancy chains (White 1970), artificial intelligence (Simon 1991, Newell and Simon 1972), linear and dynamic programming (Bellman 1984), and organization theory (Flood 1951). RAND scholars also made pioneering contributions in early space studies (Hall 1963).

Other RAND-stimulated ideas never took hold in mainstream academic scholarship but secured some attention in policy circles. For example, Leites' operational code analysis was valued by some policymakers and strategists (Marshall 1989, Schlesinger 1989). Rowen noted that Leites' 1951 book *The Operational Code of the Politburo* was "one of the most influential books to me."<sup>20</sup> However, Leites' ideas never generated much following from traditional academic scholars (George 1969). RAND also was where the intellectual foundations for net assessment were developed by Marshall (Augier 2013). Systems analysis and defense economics gathered some traction in academic circles but never made an appreciable impact on the core of economics.

The imposition of secrecy at RAND occasionally allowed deviant work that was not exposed to serious academic criticism to proceed. The result was a certain tendency toward sustaining ideas of limited merit, but it also shielded possibly useful deviant ideas from premature scorn. As has been observed in several contexts, nurturing novelty often requires protective boundaries around new ideas, boundaries that allow both brilliant deviance and nuttiness to resist the forces of conventional knowledge. Most organizations are more responsive to the costs of protecting nuttiness than they are to the benefits of protecting brilliant deviance. The costs of nuttiness are real, however, and the (early) RAND strategy of openness to ideas incurred the undoubted cost of protecting some inferior ideas from rigorous review.

Sometimes there was a tension between a culture of free inquiry and Air Force clients. For example, one of the early projects that engaged Rowen at RAND (under Wohlsetter) was the Strategic Base Study (1951–1954). The team was to identify the important factors in strategic air base selection and evaluate alternative basing systems according to performance. The study used systems analysis and focused on cost effectiveness/economic efficiency arguments. The decisions examined involved trade-offs between factors such as logistics, proximity, and relative vulnerability to enemy attack. The study concluded that the Air Force's strategy had significant weaknesses and put the United States in danger of a Soviet preemptive surprise attack. A few years later, a classified study by Enthoven made similar conclusions. These studies were clearly facilitated by RAND's intellectual permissiveness, but they antagonized important elements of the institution's Air Force clientele.

### Recruitment of Intellectual Cronies

Groups of intellectual deviants were gathered together by exploiting friendship associations and other personal relations. Persons who had previously been known to each other and who had been recognized as being smart, energetic, and imaginative were recruited on the basis of previous associations. For instance, Enthoven recalls how he was invited by Rowen to speak with Hitch, the (then) head of the economics department, who then invited him to work at RAND (only a few years earlier, Hitch had also invited Rowen to work there based on a personal introduction to Rowen by RAND engineer Augenstein, who knew Rowen's wife).<sup>21</sup> Hitch himself had been recruited by Williams, who he knew from Oxford. Williams also was responsible for hiring Helmer to work at RAND (and before that, he hired him to work for the Statistical Research Group). Marshall was hired by Goldhamer based on the recommendation of Wallis, with whom he had worked at the University of Chicago. Arrow was hired on the basis of recommendations from Wallis and Girshick. Alchian was hired by Wallis, who was one of his thesis advisors. Alchian, in turn, was

in charge of hiring several other economists, including Enke (who had just left the University of California, Los Angeles (UCLA)).

Leites was hired by Williams after having served with the Office of War Information and the Foreign Broadcast Intelligence Service during the war, where he had met Speier. He also knew Lasswell, who was involved in the early social science conference at RAND. Kahn was hired based on recommendation by Samuel Cohen, a former Los Alamos physicist whom Kahn had known since high school. Wohlstetter, another early person, was recruited when he, “in a stroke of luck,” ran into Girschick, Helmer, and S. C. McKinsey on the street in Santa Monica, where he was intending to set up a factory.<sup>22</sup> Most (if not all) of these informal hires would have difficulty making it through a standard hiring review today in a big organization such as RAND. It was a mix of individual connections with evolving institutional linkages including a connection with the UCLA economics department (e.g., Enke, Alchian, Hirschleifer) and the University of Virginia (Schlesinger came from Virginia to RAND where he was assigned to be Marshall’s assistant).

The process is one that can, of course, lead to groups of friendly incompetents; but since the criteria of “belonging” were criteria that emphasized intelligence, imagination, openness, and the willingness to work hard, crony recruitment proved to be an effective route to creating a group at RAND that had impatience with mediocrity and worked very hard (Williams 1962, p. 5). Discussing what attracted people to the hot groups at Carnegie and MIT, Leavitt noted the attractor that innovative environments can be. He said, “Why did people want to stay at those places and others want to join? Certainly not because Cambridge and Pittsburgh had mild winters . . . . It was the pull of the work and the accompanying sense of all-out involvement, with others, in something active, fast, innovative, and worthwhile . . . . Periods in our lives that stretch us beyond our imagined limits are almost irresistible. It’s much like being in love” (Leavitt 1996, p. 291).

The recruitment of intellectual cronies facilitated collaboration by ensuring the mutual trust that sometimes requires years of talking and socializing. The RAND clusters rather quickly exploited their prior acquaintanceships to produce remarkably effective heterogeneous work groups. The informal, personal style of recruitment found at RAND in the early days was also characteristic of other centers of intellectual outliers. For example, Simon recalled the informality of staffing decisions at the Graduate School of Industrial Administration of the Carnegie Institute of Technology (see Augier 2001). Wallis reflected on the extent to which the hiring practices at the Statistical Research Group (Wallis 1980, pp. 324–325) worked through friendship and collegial networks: “Recruiting was by the old-boy network pure

and unabashed: no advertising, no competitive examinations, and no attention to race, sex, age, physical handicap, or apparent nationality or surname” (Wallis 1980, p. 325). Modern examples include many start-ups in the computer software world and the creation of clusters of “hackers.”

### Facilitation of the Combinatorics of Variety

RAND thrived by mixing backgrounds, talents, and disciplines in ways that were not characteristic of more traditional centers. A RAND conference in 1947 intended to explore the organization’s involvement with more social sciences included sociologists (e.g., Speier, Herbert Goldhamer, Bernard Berelson, Frederick Mosteller), psychologists (e.g., Donald Marquis, Ernst Kris), anthropologists (e.g., Ruth Benedict), economists (e.g., Hitch, Wallis, Alchian, Jacob Viner), political scientists (e.g., Lasswell, Leo Rosten), and mathematicians (e.g., Weaver) in addition to the usual suspects. Herbert Goldhamer remarked during the conference that before this effort to get into social science more seriously, “RAND had been tentatively circling around a rather vast ocean and getting its toes wet” (RAND 1948).

In his opening remarks to that conference, Weaver observed, “One of the nice things about this job [at RAND], at least one of the things that I am awfully happy about, is that you let somebody, whose background is originally that of an engineer and secondly that of a mathematician, into a room with . . . a number of social psychologists and political scientists and economists. That is, I think, an indication of the direction in which we are moving” (quoted in RAND 1948, p. 9). Williams (1961) saw RAND as an institutional device for “overcoming the increasing compartmentalization and specialization of knowledge” (p. 2); and Lindblom noted that with regard to interdisciplinary interaction, RAND was better than universities: “It was a real community of ideas.”<sup>23</sup>

As in most adaptive systems, a primary source of novelty in ideas at RAND was the combination of disparate elements. In many ways, combinations were dictated or at least informed by the problems addressed (Simon 1986; see also Williams, quoted in RAND 1948). The problems of warfare and security did not reliably respect disciplinary boundaries (Williams 1950). It was always challenging to get disciplines to talk together, but the focus on problem-driven research led naturally (if not inevitably) to an emphasis on combining multiple disciplines in pursuit of possible solutions. This was partly because urgent problems made disciplinary minds less “territorial.”<sup>24</sup> Wohlstetter mentioned the importance of people being “seduced by a problem”—seduced away from their “normal” disciplinary background to interact with others with different perspectives.<sup>25</sup> At the same time, tendencies toward interdisciplinarity were abetted by an emerging behavioral science perspective



that encouraged multidisciplinary explorations throughout the social sciences. The mood of the times was both reflected and influenced by the number of interdisciplinary committees and groups around institutions such as the University of Chicago and the Ford Foundation (Augier and March 2011, Chapters 4 and 6). Whereas they differed in focus, they were united in emphasizing how multiple disciplinary perspectives could help to solve contemporary problems and to assist in the development of social science.

The problems that RAND addressed were to some extent inherently multidisciplinary, but there were also more mundane factors that contributed to the effective combinations. First among these was the intellectual freedom of the institution. As mentioned earlier, RAND provided a context of considerable scholarly freedom. Both the organizational hierarchy and the intellectual hierarchy were deemphasized. Nobel Prize winners were not given automatic precedence over graduate students in discussing ideas. Similarly, at the Graduate School of Industrial Administration of the Carnegie Institute of Technology, students worked alongside professors; ideas mattered more than titles. The RAND welcome pamphlet for employees in 1957 proudly trumpeted the organization's lack of hierarchy in the note "Welcome to RAND":

You may have noticed already that the lines of authority are not as rigidly drawn here at RAND as is usual in most organizations. We like it that way, and we hope you will too. The fewer controls the better, we think, and the more each individual acts on his own responsibility the better. Maybe one reason why this atmosphere prevails is that RAND's work calls for originality and initiative; our products are ideas, and neither the ideas nor the way of producing them fit into repeating patterns.<sup>26</sup>

The perceived urgency of the problems facing the country seemed to make disciplinary minds less rigid. Williams was a key force in breaking down disciplinary boundaries. He himself had been trained in theoretical astronomy but had also studied mathematics at Princeton before heading the Applied Mathematics Panel's Statistical Research Group at Columbia working on operations research problems in battles. Initially fascinated by military worth and mathematical theories of warfare, he was also early to recognize the need for other disciplines.

Williams was less concerned about the possible frictions between the disciplines or their internal hierarchy than the need to be able to understand real-world issues in all their complexity and detail; nor did he find his own background limited by his own embrace of other perspectives. His vision extended to the social sciences. In 1946, he put together a group to discuss military worth as well as to build up skills at RAND with scholars such as Wallis (economics), Herbert Goldhamer (sociology), and Rosten (sociology), and he also recruited

scholars such as Mosteller and Helmer. Williams' enthusiasm for broadening the disciplines involved was evident in the 1947 social science conference as well. He asked anthropologist Mead to participate in an early project. Williams, in looking back, recalled that she had a "shocked look on her face" when "she learned that working for RAND involved working for the Douglas Aircraft Company" (Bornet 1962, p. 34). Mead, however, did agree to work for RAND.

In the early days of RAND, architecture and the problem focus facilitated the mixing of disparate talents. RAND was built around patios that served as meeting places for individuals who would not ordinarily meet; the design of the buildings inviting interdisciplinary and unusual contacts. A key architect behind this arrangement was Williams, who was well aware of the importance of physical context for intellectual content: "RAND represents an attempt to exploit mixed teams, and that to the extent its facility can promote this effort it should do so. This implies that it should be easy and painless to get from one point to another in the building; it should even promote chance meetings of people" (Williams 1950, p. 7).<sup>27</sup>

Underlying the importance of such chance meetings was an awareness of the way organizations can facilitate interpersonal conversations: "I think one of the least publicized facts about human organization is the extent to which personal contacts are important—particular to successful organizations—and the extent to which one's personal knowledge and the confidence one reposes in a person is transferable" (Bornet 1962, p. 30). Thus the RAND office buildings were explicitly designed to keep people mixed in unusual ways. Offices were oriented to patios in the inside of the buildings rather than outside corners, somewhat reversing the prestige of outer versus inner offices. Williams (1950) observed, "If a multiple patio scheme were artfully done, it would develop that 'outside offices' [i.e., the ones overlooking the beach] in the normal sense, would rank low in popularity" (p. 7).

The power of architecture and organizational structure on interaction is seen in the exceptions. Some of the work that physicists did required a variety of security clearances that necessitated a separate door between their work area and the rest of the organization, thus minimizing interaction. The social science department was first located in Washington, DC, away from the main headquarters of RAND in Santa Monica. Its interactions with other disciplines were small until it relocated to Santa Monica.

Physical proximity is important, but it is not enough. The generation of contact is inhibited by a very general mechanism of variety limitation—homophily, the tendency of elements to congregate with other similar elements. Homophily operates at two levels: First, an institution recruits and retains individuals who are similar to each other in training, experience, and beliefs.



Second, individuals within an institution establish and maintain workgroup contact with people similar to themselves more commonly and more easily than with dissimilar people. Homophily at either level serves to discourage the combination of disparate elements. If an institution as a whole has little variety, any mixing rules in the formation of workgroups will bring similar elements together in workgroups. If an institution as a whole is heterogeneous, homophilic formation of workgroups will tend to result in workgroups without internal variety, and thus meager combination of disparate elements.

RAND seems persistently to have combined disparate elements within workgroups. The combination of disparate elements resulted in part from the fact that (for some reason) a relatively heterogeneous pool of intellectuals was recruited to the institution. At least in the case of RAND, the intention was to get the best minds trained in the disciplines, then induce them to work across disciplinary lines, at least to a greater extent than normal, often because the nature of the problems on which they worked did not fit a single discipline—in other words, RAND was multidisciplinary by design (see RAND 1948). And it happened in part because the process of workgroup formation was incompletely homophilic, or homophilic with respect to a different attribute (e.g., recreational preferences).

The pressures toward intellectual homophily did not disappear. Given a choice, most of the scholars residing in these centers gravitated toward others who shared their training, inclinations, ideas, and worldviews. Homophily undermined the intellectual culture at RAND. Helmer commented on the latter-day difficulties at RAND in getting dissimilar people to work together as they had in the early days:

What I liked very much about the early days at RAND ... was the spirit of cooperativeness and openness... which was fostered very largely through John Williams ... he was instrumental in bringing in economists and political scientists. But much to our distress, as time went on, we found that among particularly the political scientists, there was much more of the sort of academic relative secrecy among them. They did not welcome the idea, as much as we had done, of cooperating with people in other disciplines. They were not intrigued with the possibility of a multidisciplinary approach to problems.<sup>28</sup>

Tendencies toward homophily were affected by features of the organization—partly deliberate, partly not. In general, the smaller an institution in terms of the number of individuals involved, the harder it is to maintain homophily in voluntarily forming subgroups. Large size in the former facilitates homogeneity in the latter. If you have 20 people representing six disciplines, it is hard to find many groups of five that are monodisciplinary. If you have 200 people representing six disciplines, it is not hard to find groups of five that are monodisciplinary.

Some of the key projects at RAND manifestly demanded knowledge drawn from several disciplines, but even where the requirements were ambiguous, the centers often proclaimed, and to some extent insisted on, an ideology that defined the problems on which they worked as necessarily multidisciplinary. Physicists could prefer to work with other physicists, but they could not declare that all problems of interest were problems that involved only the knowledge of physics without encountering some elements of social disapproval.

As RAND grew, the importance of the physical architecture for RAND became evident, as noted by Specht (1958, p. 2):

As John Williams said in 1950, “RAND represents an attempt to exploit mixed teams, and to the extent its facility can promote this effort it should do so.” That is, at RAND, much more than at a university, a physicist is apt to encounter the political scientist, the engineer to consort with the economist. This is true—and important—not only in the formal work of an interdisciplinary project team, but also in the many internal contacts, ones the building design should stimulate. An expert in international relations may write a book by himself, but he is a different man and it is a different book because he has been simulated and educated by encounters with colleagues of many disciplines and varied experience.

In the early years, both RAND and the workgroups within it were small in numbers. The combinations produced by interactions among a variety of different people were encouraged by smallness. Because of the small size, the ideology, the problem foci, and the work arrangements, scholars worked, lunched, and had offices in arrangements and across projects that led to numerous casual contacts that violated homophilic rules.

### Speculating About the Seeds of Decline

RAND exhibited a period in which exceptional clusters of intellectual imagination thrived, followed by subsequent decline. To some extent, of course, such fluctuations are simply one more instance of regression to the mean. The random processes that produce outliers necessarily produce a tendency toward their subsequent decline from earlier intellectual excitement.<sup>29</sup>

Regression to the mean is a necessary product of random sampling, but it is not a necessary product of behavioral causality. When behavior is taken into account, decline is not a necessary aftermath of success. There are numerous situations in life in which human behavior results in the amplification of random events. For example, early scientific successes color the assessment of subsequent contributions by the same scientists. Individuals who are described as leaders have their behaviors subsequently seen as leader-like more often than those not so labeled. People who are successful develop self-confidence that (under some circumstances) makes

future success more likely. In the present case, it would not be surprising to discover that individuals and organizations that are recognized as producing successful intellectual outliers might subsequently be seen indefinitely as leading sources of innovation. They might attract scholars who longed for challenge and high-variance alternatives for their research. Reputations might feed on themselves.

Similarly, although regression to the mean is undoubtedly a factor, there may be behavioral and organizational mechanisms that either moderate or contribute to decline. On the one hand, a reputation for positive outliers facilitates the recruitment of talent. At the same time, however, it appears to be true that the flaring of intellectual outliers involves the stimulation of intellectual variety, but successful variation inhibits the further generation of variety. Successful intellectual outliers become the basis of a new intellectual orthodoxy. Variation is self-defeating, particularly when its success leads to hubris and arrogance, and arrogance has been reported as typical in many groups that have produced intellectual outliers (for example, [Leavitt 1996](#)).

We consider three major organizational consequences of achieving success with intellectual novelty: success yields a sense of competence, it yields endurance in a competitive world, and it yields the opportunity and inclination to grow. Self-confidence, endurance, and growth produce numerous positive consequences for an organization, but for the most part, they undermine variety. Since success produces all three—self-confidence, endurance, and growth—it is not easy to separate the variation reduction effects of one consequence from the effects of the others; but collectively, they tend to be overpowering.

### Self-Confidence

The self-confidence that comes from success reflects the attribution of outcomes to personal or organizational qualities. Good outcomes that are due to some mix of capabilities and luck are attributed primarily to the former or to some personal access to luck. As a result, success leads to overconfidence and to limitations on self-criticism ([Lindblom 1959](#), [Whitehead 1925](#)).

Overconfidence and limitations on self-criticism have positive effects on variety insofar as they lead to an underestimation of risk. Successful managers are inclined to take risks because they have learned (in part, erroneously) that the risks do not apply to them ([Kahneman and Lovallo 1993](#)). However, success leads not only to overconfidence about risks but also to overconfidence with respect to the actions that have been associated with success. Successful actors tend to repeat actions that were associated with past successes and to avoid other possibilities ([Denrell and March 2001](#)). They become seduced by invitations to give advice, invitations that lead them to place greater emphasis on usefulness than on creativity.

As RAND members became convinced of the dangers of the international situation and the usefulness of their ideas, they sought influence: “Faced . . . with a feeling of emergency in national defense policy, if not sometimes with a foreboding of catastrophe, many RAND members want[ed] a more immediate and comprehensive kind of influence” ([Lindblom 1959](#), p. 16). However, seeking influence tended to undermine the bases of influence. Among other things, seeking influence leads to biases and narrowness of minds, which itself undermines real influence; few, however, can resist the desire to be “relevant” and make policy decisions (as opposed to understanding decision making in policy situations). In addition, involvement in making decisions inevitably leads to sharing sentiments with decision makers, thereby reducing the autonomous role of independent thinkers.

The importance of understanding decisions, not making them, is also emphasized by [Williams \(1961\)](#): “It is important that RAND’s role in government be that of advisor at most; much of it is in fact that of precursor and catalyst . . . it must not accept managerial and decision making responsibilities” (p. 10). Wohlstetter felt that RAND declined because it became more like a consulting firm and less like an intellectual institution with subsequent decline in “its willingness to take risks, and its disposition to think up alternatives that were likely to meet opposition at a time when ideas are tentative and vulnerable.”<sup>30</sup> Relevance, of course, also leads into the pitfalls of public policy and politics. When key members of the RAND global strategy elite (e.g., Hitch, Rowen, Enthoven, Ellsberg) became part of the Kennedy administration—involved in not only politics but Democratic politics—it led to disquiet at RAND and among its Air Force sponsors.

Part of this may have to do with the degree to which researchers interacted with their “customers.” [Lindblom \(1959\)](#) argued that basic interdisciplinary, empirically driven research seems to be most influential when it does not seek influence, most powerful when it does not seek power. He noted that if one seeks influence on policy, then one is more likely to ignore, consciously or not, contributions from disciplines and perspectives other than one’s own and less likely to understand the issues. It was a sentiment echoed in other studies of innovative centers of research ([Gehani 2003](#), p. 50).

Self-confidence leads to replicating specific successful actions, not a strategy of variety that led to discovery of those actions. At RAND, for example, confidence in systems analysis led to a narrowing of minds; increasing unwillingness to understand the full complexity of the problems that were supposed to be illuminated; and, over time, an arrogance that the conventional method could apply to most, if not all, problems of the world ([Lindblom 1959](#)).

It is worth pointing out that the more thoughtful proponents of systems analysis were aware of the limitations of the approach. Hitch was one of the early

pioneers for systems analysis but was also very articulate about its limitations (Hitch 1955, 1958, 1960; Koopman and Hitch 1956). Others who warned early about possible limitations included Herbert Goldhamer, Alchian, Marshall, and Schlesinger (Alchian and Kessel 1954, Alchian et al. 1951, Goldhamer 1950, Schlesinger 1973). They argued that a blind application of systems analysis without more systematic research on human behavior was likely to lead to mistakes and that, as a result, the tools and methodologies that may have helped illuminate some decision-making processes in the national security arena could not be unthinkingly extended to unrelated areas (Schlesinger 1973). Goldhamer (1950) noted in his paper on the “human factors in systems analysis” that the systems analysis perspective was flawed without incorporation of research on human behavior. He launched efforts in that direction, which ultimately became the Systems Development Corporation. Other attempts to include more research on human behavior included work that Newell and others did on man–machine interactions; others centered around a systems research laboratory headed by psychologist John Kennedy.

To some extent, RAND was torn between an enthusiasm for systems analysis and a concern that researchers would be expected to make it work in situations in which they did not understand how to execute it. Both horns of the dilemma sustained a narrowing of intellectual attention. The hubris about early discoveries led to a tendency to diversify into fields for which the techniques were less useful and with which RAND was less well equipped to deal. RAND in a sense had developed its own “competency trap”; it had become good enough at systems analysis that analysts used the method in situations in which alternative approaches (if similarly developed) would have served them better. Later disciples of systems analysis tended to overlook the criteria, which had been central to Hitch’s original vision for systems analysis (Hitch 1953). When Lindblom was invited to RAND in 1954 to study the criterion problem, he grew increasingly frustrated with the way the strong adherence to systems analysis biased the research. The systems analysis perspective became influential in the McNamara era of defense planning and budgeting in America, although it (especially later) strayed quite far from Hitch’s original vision and became detrimental to the development of strategic thinkers despite its earlier contribution to it (Marshall 1991). The (perceived) success in systems analysis in the defense area stimulated attempts to push and apply the methodology to other areas as well. At the same time, worries about a possible oversell of RAND’s capabilities in systems analysis led to a focus not on new ideas but on refining and implementing old ones.

## Endurance

The endurance that comes with success leads to aging. The impact of aging on organizations is well documented (Aldrich and Auster 1986, Lumsden and Singh 1990). Aging often has positive effects on mean performance, but it has few positive effects on variety. Enduring bureaucracies emphasize reliability through rules, recruitment, and regulations. Aging organizations accumulate procedurally reliable personnel. Over time, ambitious and talented individuals leave an organization, and a successful organization fails to eliminate the untalented and unmotivated, leading to the accumulation of “deadwood” and a steady increase of red tape (DeWeerd 1959, Lindblom 1959). Aging organizations develop routines and stability in employment that are inimical to variety. Offices, divisions, relationships, and authority tend to persist.<sup>31</sup> By contrast, a young organization, though hobbled by a lack of knowledge and experience, attracts and cultivates flexibility, energy, and vigor.

The growth in the number of administrative personnel relative to researchers was noted in RAND management committee meeting notes. A report to the trustees in 1963 mentioned that, if each employee is classified as either a researcher or a part of the administrative staff, between 1951 and 1963, the proportion of researchers had fallen from 51% to 39%, with the administrative staff increasing from 49% to 61%.<sup>32</sup> Lindblom’s (1959) analysis of RAND, for example, warned that one of the trends pointing to decline was the rise of an “administrative class,” which pursued neither the long-term interests of the organization nor the development of science and ideas that researchers sought. DeWeerd’s (1959) analysis is shorter but reaches similar conclusions, especially about the increased presence of red tape that he found. Writing about the attitudes of the RAND administrative class toward the difficulties at RAND, Lindblom observed, “They do not deny its decline but instead explain why the decline should be accepted” (p. 6). Augenstein noted that “bureaucratic encrustation” had begun to set in at RAND by 1960.<sup>33</sup> Goldhamer, reflecting on 25 years of experience at RAND, observed that the growth of an administrative class at RAND crowded out the creative individuals. He strongly advocated refocusing RAND around key individuals and not trying to fit individuals into areas or topics decided by administrators. These complaints about the expansion of administration with its deleterious effects on imagination tended, of course, to ignore the administrative necessities produced by RAND’s larger budgets and bureaucratic visibility.

## Growth

From 1948 to 1962, RAND grew from 225 employees and a budget of \$3.5 million annually to 1,100 employees and a budget of more than \$20 million.<sup>34</sup> In many circles, particularly managerial circles, growth is treated as



a clear marker of success (and initially, at least RAND's growth was a result of success (DeWeerd 1959, p. 14). Thus Lindblom recalled that when he presented to the RAND president his words of caution that they should probably try to stay small, he was met with the comment, "We have to grow!"<sup>35</sup> The growth that comes with success has numerous favorable consequences for performance, but it is not good for stimulating variation. By extending the organization beyond a face-to-face group, growth encourages self-interest more than group interest. It mandates efforts to elaborate rules and regulations that standardize activities and reduce variety. Growth leads to the diversification of applications of what is known, rather than diversification of solutions. Organizations tend to apply their successes to new areas rather than to generate new directions. Growth undermined a critical feature of intellectual variety—the cross-fertilization of irrelevant contacts. It facilitated specialization and a narrow focus, rather than the combinatorics of multiple viewpoints. Growth necessitated the expansion of administrative offices, which encouraged the substitution of bureaucratic ambition for technological or intellectual ambition. Growth also inhibited the practice of informally recruiting really bright, but deviant, individuals (and building research programs around their interests). The larger organization tended to hire people who conformed to conventional disciplinary templates and fitted a predefined research agenda (Goldhamer 1972, p. 1).

Thus, a major threat to RAND's uniqueness as a center for intellectual cross-fertilization and the sustenance of deviants came not from failure but from success. As the ideas associated with system analysis became accepted both within RAND and outside, intellectual deviants (e.g., Leites) had less and less contact with the core activities of the organization. For a relatively long period, such deviants continued to be acknowledged as symbols of RAND's heterogeneity and intellectual excitement, but they increasingly became more tokens of the ideology than important actors in the central intellectual life of the organization.

Growth also made more difficult the interdisciplinary collaboration that was so central in the early days. A group of staff members at RAND took note of these problems. One of their memos noted that

RAND's most unique contribution, and its greatest strength, lies in its success for handling work outside of existing specialties. To continue to accomplish such ends, however, requires maximum cooperation of RAND personnel within and between the various subdivisions of RAND. With the passage of time we see departments tending to become institutionalized, self-contained entities rather than administrative conveniences. Some even show the desires of flourishing in splendid isolation. Our concern is that such a trend can act in a manner detrimental to the successful accomplishment of interdepartmental projects and goals.<sup>36</sup>

Growth also led to the multiplication of coordinating groups, administration, and meetings. Speier noted,

Meetings of permanent bodies—committees, subcommittees, advisory boards, etc.—are part of the formal communications system. The more meetings of this kind are held in an organization over a given time, the less healthy is the organization. . . . The more meetings in which a person participates by preference or obligation, the less he is likely to do or discuss matters of intellectual importance in or outside of meetings. . . . The larger the number of persons participating in a meeting, the smaller the chance that anything useful will be achieved in settling issues.<sup>37</sup>

DeWeerd (1959) also noted that one of the signs of decline in the late 1950s was the "industrialization" of the administrative structure of RAND, as manifested, for instance, in a "steady growth of red tape," increasing status symbols for administrative staff and growing emphasis on the packaging and merchandising of research (rather than research itself) (p. 19).

In a grand sense, the processes are manifestations of a fundamental mean/variance dilemma: what is good for maximizing average performance (e.g., order, routinization, tight controls, rules, unity, homogeneity) is bad for maximizing variety; what is good for maximizing variety (e.g., disorder, spontaneity, loose controls, conflict, heterogeneity) is bad for maximizing average performance. For many good reasons, the typical small, young organization is not likely to survive; the ones that survive will tend to be the ones that have, by brilliance or luck, experimented with a good novel solution. But the fortunate ones who survive the first stage will not survive further unless they change to adopt characteristics that undermine their inclination to produce variation. An organization that continuously generates intellectual outliers is imaginable only in a world where the passions of arbitrary interest are protected from the urge to repeat success, a world that arguably would be hopelessly disoriented.

The closest thing to such an organization was probably the research university of the 20th century. Even there, the tendency to repeat previous success was strong. As universities gained recognition as centers of research, they sought increasingly to imitate one another and to repeat previous successes. By the 21st century, there was a real risk that many North American universities had become sufficiently seduced by prospects for immediately profitable collaboration with business enterprises such that they tended to abandon the real pursuit of variety and originality while espousing its rhetoric (Augier and March 2011).

The alternative to trying to imagine an organization that continuously generates intellectual outliers is to try to imagine a universe of organizations that do so collectively. Suppose we concede that success inevitably undermines variety. Then, variety has to be produced in young,



small organizations that exhibit variety for awhile before they either die (a likely event) or produce a successful deviation (an unlikely but occasional event). In the latter case, either success extinguishes their capability to generate variety or the competition extinguishes them. The community requires a continual replenishment of high-variance organizations that arise despite their negative expected value. This requires, in turn, a system that continually produces a population of potential entrepreneurs and investors who (for whatever reason) are inclined to support low mean/high-variance foolishness.

### The Engineering of Flaring

Realizing the benefits of intellectual outliers involves the engineering of enduring institutions that produce them. Historically, such institutions have been rare and short-lived. The fundamental reason for the rarity of institutions that produce outliers and for their short lives is that outliers and the conditions that produce them are not favored by their environments. Most crazy ideas are crazy, and the same conditions that produce brilliant, path-breaking new ideas produce a lot of cockeyed nonsense. As a result, it is generally not sensible to encourage outliers. And if by some chance an outlier proves to be valuable, it is generally more sensible to refine and use that idea than to seek new outliers. RAND was a gamble after the war. It was the kind of gamble that would normally be a losing one. Hitch (1958) noted that “research seems to attract an optimistic breed” (p. 3). Against the odds, RAND succeeded. Subsequently, the organization declined as a source of new ideas, in large part because it adopted policies that diminished variance. Outliers disappeared, but RAND survived. Later presidents seemed inclined to design for endurance, rather than variance.

Marshall and Schlesinger were actively engaged in trying to help then RAND President Rowen to try to reshape RAND in a direction that might (re)capture its earlier spirit. But they also were very aware of the difficulties. For example, Schlesinger wrote in a memorandum<sup>38</sup> to Rowen: “There is no good way to structure RAND.” Comparing the choice of organization either by discipline or by function or problem area, he noted, “RAND has traditionally adhered to organization by discipline. In the early days of the organization, with greater internal flexibility, this structure did not necessarily preclude a functional approach [i.e., problem-driven research]. Unhappily, this can no longer be said to be the situation.” He also emphasized both benefits (in terms of keeping up with professional developments in the disciplines) and costs: “Many RAND personnel remain wedded to their disciplines in the narrowest sense, and contribute remarkably little, to the understanding of the big problems.” In this memorandum, Schlesinger linked the narrowness to the lack of organizational flexibility:

With the erosion of internal flexibility and also the disappearance of the centripetal force represented by the universal concern with the threat posed by the Soviet

Union, RAND research efforts have become increasingly fragmented. Partly for disciplinary reasons and partly for more localized bureaucratic reasons the departments likewise have increasingly become pools of power that tend to resist attacking problems on RAND-wide basis. Much departmental effort is not relevant to RAND’s main objectives and represents only a casual effort to keep up with what is going on in the separate disciplines.

Designing for interdisciplinarity is also difficult, yet some things may help. Williams wrote, in the context of discussing RAND’s self-criticism, “Effective interdisciplinary research requires singularly intimate intellectual and personal relations. I haven’t a clue as to how to achieve it invariably, or quickly. But it is like a marriage, concerning which I know a theorem: Propinquity is a prerequisite.”<sup>39</sup>

Creative energies can emerge from the interaction of people from different disciplines—but such mixing may run contra to most academics’ natural centripetal tendency toward seeking likeminded and similar disciplinary minds. The problem is a classical one that has been noted in many contexts: people learn most from others different from themselves yet seek the company of those who are similar to themselves.

The basic lessons from the RAND experience are clear. They are also familiar. An adaptive system requires a mix of exploration and exploitation. In the short run and near neighborhood of the adaptive mechanisms, however, the returns from exploitation and the costs of exploration are greater (and more visible) than the returns from exploration and the costs of exploitation. As a result, exploitation is systematically favored by ordinary adaptation and tends to drive out exploration.

Yet there is repeated evidence that exploration is needed. In Weaver’s autobiography, he wrote,

The more serious and significant aspects of the progress of science during my lifetime could, of course, only be described in technical terms. To the trained scientist, the significant advances are likely to be improvements in complicated and abstract basic theories. These improvements give the theories greater precision, broader application, and increased aesthetic grandeur. These improvements are often too technical and too abstract to seem impressive to the non-scientist. But it is just these deep and abstract improvements in basic theories which . . . lead to the . . . improvements that touch and serve everyone. (Weaver 1970, p. 135)

Engineering solutions to this problem are of four types. The first type attempts to improve the rationality of decisions by extending the time and space horizons of decision makers so that the more distant benefits of exploration are fully recognized in the analysis. The second type attempts to lead decision makers to support exploration by providing false information about the likelihoods of positive returns from exploration. The third type attempts to buffer exploratory activities from the pressures of efficiency. The fourth type attempts to

protect exploration from analysis by connecting it to dictates of identities.

### Extending Time and Space Horizons

Research on time and space horizons in individuals has focused on two fundamental individual problems that are common though not universal.

1. *Impatience*: Individuals often seem to find it difficult to delay gratification, even under conditions in which such a delay is almost certainly rational. They prefer immediate, rather than delayed, reward, even when the delayed reward is manifestly greater. They avoid short-term pain, even when such pain is associated with large longer-run benefits.

2. *Self-centeredness*: Individuals often are reluctant to surrender rewards for the benefit of others or a larger group that includes themselves. They focus on the self rather than more extended groups and are likely to overlook the indirect, diffuse benefits of belonging.

Organizations have similar difficulties. Former Secretary of Defense (and before that, research scientist at Los Alamos and Livermore Laboratories) Harold Brown mentioned the “tyranny of the inbox” and the need to be able to lift the perspective above day-to-day events and “current problems” and have a longer perspective (Brown 2012, pp. 16–17). He cited the need for organizations to have a part or fraction of its best people be devoted to long-term trends and issues.

The locus of organizational action is often more local in time and space orientation than are the effects of action. As a result, organizations tend to be myopic (Levinthal and March 1993). Modern efforts to extend time and space horizons of decision makers generally seek to link incentives to distant horizons, for example, through providing stock options that are realized only in the long run. The results are mixed. On the one hand, there appears to be little question that the possibility of enormous future return motivates technological inventors and entrepreneurs. On the other hand, there is some question about the extent to which it motivates them to engage in fundamental research or in projects having indefinitely longtime horizons, and whether such motivation reliably produces technological novelty.

### Falsifying the Expected Returns

Most of life involves denying the reality that in the long run, we are all dead and the species is extinct. It involves constructing hopes in successes that are unlikely to be fulfilled, embracing the triumphs of history as the central story of history. Many more books are written about the successes of research or business than are written about the failures; yet most research yields modest results and most businesses fail. Optimism about science was an important aspect of RAND in the early years, but the glories of progress celebrated in the mythic stories of science are rarely replicated in research.

The falsifications come in two major ways. First, they are embedded in deep beliefs about the long-run return to science. There are very few basic research scientists who fail to accept and recite some version of the litany summarized in Flexner’s glib proclamation about the usefulness of useless knowledge. They can easily offer examples. However, it is a rare scientist who seeks to gather any systematic data on the expected return to fundamental research.

Second, they come from the traditions of reporting scientific results. Positive results secure much more attention than negative ones, particularly when the positive scientific results are also connected to positive practical consequences. The telling of scientific history is profoundly biased by confidence in scientific success.

### Buffering Exploration

Because its returns are uncertain and usually located at a spatial and temporal distance from current activities, exploration tends to be vulnerable to efforts by organizations to maximize expected return. To survive and provide the more distant and less certain returns that it offers, exploration needs to be buffered from the ordinary organizational processes of efficiency seeking. Excess resources (“organizational slack”) sometimes provide a buffer, a buffer that inevitably pays the price of protecting the indolent and malevolent as well as the imaginative, but it may, nonetheless, be useful (Enke 1967).

When writing to Oppenheimer regarding the organization of research at the Institute for Advanced Studies and the recruiting of researchers for positions there in 1948, von Neumann expressed a need for trust as a buffer protecting research:

I feel strongly that the basic principle should be this: A research appointment is essentially a position of trust, expressing the belief of the appointing group in the appointee’s ability and desire to do productive research... such an “expression of faith” should not be a priori combined with strict legal and administrative safeguards of its fullness or attempts at such safeguards... I think the primary approach ought to be one of nonintervention and of trust.

(quoted in Rédei 2005, p. 191)

RAND itself was such a buffer within the defense establishment, a buffer that sought to make exploratory work possible by making it invisible to the mavens of bureaucratic efficiency. As RAND grew, there were still occasional efforts to create some kinds of small “skunk works” that were protected from normal bureaucratic controls and pressures. For example, a strategic studies initiative (first headed by Schlesinger and subsequently by Marshall) was initiated. It was intended to have its own budget and hiring processes, and it was created to focus on some of the broader strategic issues that RAND had focused on early on, in its most successful

years (Enke 1967, p. 3). The effort attempted to shelter from normal bureaucratic procedures a small amount of research focused on such issues.

### Substituting Identities for Incentives

Both by modern ideology and by experience, organizations are creatures of incentives. Activities within an organization are motivated and coordinated by the granting and withholding of incentives. The manipulation of incentives is a primary tool of managerial direction. Insofar as research workers are sensitive to incentives controlled by management, management can control the direction of research. Managerial control of research ordinarily disadvantages exploratory research because managerial incentives tend to emphasize immediate and local returns. It is not that managers are malevolent but that they are efficient. Research that is predicated on managerial incentives will in numerous ways undermine exploration.

Exploration protects itself from managerial incentives by embedding itself in the demands of a research identity. The scholar is a scholar and does what a scholar should do. Fundamental exploratory research that is serious is conducted for its own sake and in response to a claim of a scientific identity. One of RAND's most fundamental disadvantages lay in its long-run effort to exercise managerial incentive control over its research, an effort that undermined the position of the norms of autonomous research. This effort necessarily made RAND less attractive to employees with strong fundamental research identities. RAND increasingly recruited very smart people whose motivations were incentive based rather than identity based—thus people who were less inclined to seek autonomy than to seek ways to satisfy their masters, people who sought financial rewards and careers rather than contributions to fundamental knowledge.

### Closing

It is hard to learn from a single case, particularly when most of the key features of the case are quite consistent with the idea that the intellectual flaring observed was simply a random anomaly and particularly when it is hard to be entirely confident that the regularities we observe are regularities in the phenomena rather than simply shared prejudices of observers. Nevertheless, we think some modest clues to the occurrence of intellectual outliers can be extracted from the story of RAND.

Flaring seems likely to be produced by the interaction of three distinct kinds of processes. The first is the independent, simultaneous generation of ideas through the imagination of individual scientists. Discovery is a highly uncertain, low-probability process, so the simultaneous realization of discoveries by multiple scientists in the same neighborhood is an extremely unlikely event if

the discoveries are independent. It is conceivable, however, that the likelihood of discovery by individual scientists may be responsive to organizational conditions.

The second kind of process is the combinatoric contagion of discovery, the way in which imagination in one scientist is stimulated and transformed by contact with the imagination or knowledge of suitably adjacent others. Under such circumstances, discoveries are not independent, and the likelihood of flaring increases. Organizations may encourage or discourage such contagious processes, primarily by facilitating or inhibiting contact among individual scientists.

The third kind of process is the emergence of cultures of imagination, clusters of scientists bound together in an ethos that expects and demands imagination. The way in which cultures emerge from the history of a group is not well understood, but it involves interaction, consciousness of distinctiveness, and the formulation and spread of mythic history.

All three processes seem to have been involved in some parts of RAND in its early years, particularly in the economics and mathematics departments. The organization encouraged autonomous imagination, interactions that produced combinatoric contagion of ideas, and the elaboration of myths of imaginative excellence. The encouragement was, to some extent, conscious, reflecting the judgment of leaders that all three needed to be stimulated.

At the same time, the phenomena appear to have been consequences of both a specific context and a specific historical flow of events that were not in particular products of managerial intent. RAND created neither the postwar milieu of patriotic optimism nor the explosion of interest in a scientific social and behavioral sciences that profoundly shaped the RAND of the 1950s. This context surrounded many institutions without producing significant flaring of intellectual outliers, so there apparently were features of the RAND response to the context that facilitated the flaring; but without the context, it seems unlikely that such flaring would have occurred.

One of the more intriguing and puzzling features of the RAND experience was the combination of a general sense of compelling and urgent social needs along with the encouragement of intellectual autonomy and freedom. The intense demands of the Cold War upon the Air Force and upon strategic planners in the larger defense establishment were a pervasive feature of life at RAND. This was combined, however, at least in some parts of RAND, with an equally pervasive pressure toward the independent autonomy of individual scientists or groups of scientists. RAND, particularly in the economics and mathematics departments, implemented one of the oldest and most difficult to implement axioms of imaginative management: insist on enthusiastic commitment to the objective but encourage free play of independent imagination in discovering routes to that objective.



There were few people at RAND who questioned that the Cold War was real or who denied the importance of the United States winning that war, but they varied considerably in the ways they formulated that problem and in the solutions they imagined; that variance in possible means was encouraged by the same authorities who demanded wholehearted acceptance of the goals. In some respects, it was the shared sense of compelling goals that made the autonomy possible. Tolerance for independent ideas was built on a strong sense of shared fundamentals. Deviant ideas were protected by their authors' belonging to a community of shared objectives. When later in RAND's development, the objectives became less clear and less shared, deviance became less tolerable.

Outbursts of new ideas are not "natural." They violate the strong tendencies of knowledge to feed on itself, rather than outside sources, and the strong tendencies of knowledge carriers to cluster with similar others and to pursue personal advantage, rather than gains to knowledge. Outbursts of intellectual outliers occur when these tendencies are thwarted by combinations of factors difficult to anticipate though modestly susceptible to control. The outbursts produce experiences of personal joy for those involved, followed by dismay that the experiences were transient. Participants look for secret incantations that will reproduce the magic but realize at last that their contribution to the magic may have been more ephemeral than they might wish. It is a lesson hard to learn but possibly worth learning, for it teaches that the wise manager may prefer to predicate innovation less on hopes for its consequences than on an attachment to the obligations of an identity.

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### Endnotes

<sup>1</sup>Intellectual history often tends to be written in terms of individual thinkers who were outliers and factors in their individual histories that produced novelty. Without denying the importance of individually idiosyncratic factors, our focus is on organizational features that recruited, produced, or shaped clusters of such individuals.

<sup>2</sup>Interview with Lloyd Shapley, February 9, 1994, National Air and Space Museum Archives, Washington, DC, p. 12.

<sup>3</sup>Interview with Albert Wohlstetter, July 29, 1987, National Air and Space Museum Archives, Washington, DC, p. 5.

<sup>4</sup>Letter from Allen Wallis to Frank Collbohm, dated April 10, 1947, W. Allen Wallis Archives, University of Rochester, Rochester, NY.

<sup>5</sup>Interview with Gustav Shubert, May 20, 1992, National Air and Space Museum Archives, Washington, DC.

<sup>6</sup>Interview with Albert Wohlstetter, July 29, 1987, National Air and Space Museum Archives, Washington, DC, p. 2.

<sup>7</sup>Interview with Olaf Helmer, June 3, 1994, National Air and Space Museum Archives, Washington, DC, p. 7.

<sup>8</sup>Frank R. Collbohm, excerpts from statement before Military Operations Subcommittee, House Committee on Government Operations, August 6, 1962.

<sup>9</sup>Interview by Joan Goldhamer with Nathan Leites, July 10, 17, and 18, 1986, p. 14. (A copy of this was kindly provided to us by Joan Goldhamer.)

<sup>10</sup>Interview with Albert Wohlstetter, July 29, 1987, National Air and Space Museum Archives, Washington, DC, p. 9.

<sup>11</sup>Interview with Charles Lindblom, April 9, 1991, National Air and Space Museum Archives, Washington, DC, p. 6.

<sup>12</sup>Interview with Charles Lindblom, April 9, 1991, National Air and Space Museum Archives, Washington, DC, p. 8.

<sup>13</sup>Interview with Lloyd Shapley, February 9, 1994, National Air and Space Museum Archives, Washington, DC, p. 13.

<sup>14</sup>Interview with Lloyd Shapley, February 9, 1994, National Air and Space Museum Archives, Washington, DC, p. 8.

<sup>15</sup>Interview with Lloyd Shapley, February 9, 1994, National Air and Space Museum Archives, Washington, DC, p. 8.

<sup>16</sup>Interview by Jim Digby and Joan Goldhamer with Ed Barlow, 1986, p. 2. (A copy of this was kindly provided to us by Joan Goldhamer.)

<sup>17</sup>Interview with Bruno Augenstein, January 9, 1987, National Air and Space Museum Archives, Washington, DC, p. 38.

<sup>18</sup>Similarly, key individuals such as Kurt Lewin and Douglas McGregor (at MIT) and G. Leland Bach (at Carnegie) had great influence. As Leavitt noted about Bach, "He brought in, sheltered, and supported bright mavericks, many of whom were 'too difficult' for their previous institutions and too unbusinesslike for their business community" (Leavitt 1996, pp. 291–292).

<sup>19</sup>Letter from Edwin Paxon to John von Neumann, RAND Document D-63, October 6, 1946, RAND Corporation Archives, Santa Monica, CA.

<sup>20</sup>Interview with Henry Rowen, 2012, GSB/Hoover Oral History Project, Stanford Graduate School of Business, Stanford University, CA. (A copy of the transcript was kindly provided to us by Paul Reist, director of the GSB History Project.)

<sup>21</sup>Authors' personal communication with Alain Enthoven (2013) and with Henry Rowen (2013).

<sup>22</sup>Interview with Albert Wohlstetter, July 29, 1987, National Air and Space Museum Archives, Washington, DC.

<sup>23</sup>Interview with Charles Lindblom, April 9, 1991, National Air and Space Museum Archives, Washington, DC, p. 24.

<sup>24</sup>Authors' personal communication with Alain Enthoven, October 29, 2012.

<sup>25</sup>Interview with Albert Wohlstetter, National Air and Space Museum Archives, Washington, DC, p. 16.

<sup>26</sup>RAND Welcoming Pamphlet, 1957, RAND Corporation Archives, Santa Monica, CA, p. 1.

<sup>27</sup>There is now an expanding literature on the links between work environment and creativity (see, e.g., McCoy 2005 for an overview and discussion of some of the issues), using social psychology ideas to study the importance of the physical work environment on (mostly) team creativity. Williams' short but very perceptive discussion did not go into the group dynamics



of social interaction, but he might have been aware of the earlier work by Leon Festinger and others on how people tend to associate with those with “attractive” attitudes and interests.

<sup>28</sup>Interview with Olaf Helmer, June 3, 1994, National Air and Space Museum Archives, Washington, DC, p. 18.

<sup>29</sup>The discussion of “decline” here is not meant to say that RAND in many ways was not (and still is) successful as an organization, and it continues to exist. But from the point of view of the earlier excitement, there was a decline relative to the immense flaring previously. Even Rowen, who took over as president in 1967, noted in a letter to (then) Secretary of Defense Robert McNamara his concern about the “quality and scope” of the RAND research and that it “no longer enjoyed the lead it once had” (Harry Rowen, letter to Robert McNamara, April 6, 1967 (a copy of the letter was kindly provided to us by Henry Rowen)). Thus when we speak of *decline*, it is in relation to an earlier remarkable level of intellectual excitement and capacity, both possible elements of a larger and more complex story of organizational adaptation and change in (and of) RAND one could tell if focusing on the organizational and societal issues over the last (almost) seven decades.

<sup>30</sup>Interview with Albert Wohlstetter, July 29, 1987, National Air and Space Museum Archives, Washington, DC, p. 23.

<sup>31</sup>Robert Merton noted similar issues in the development of social norms, with institutionalized practices becoming self-contained, forgetting the original purposes and adherence to the institutional norms that made up the ritual (Merton 1938).

<sup>32</sup>Memorandum sent by Robert Specht, “Notes on some patterns of change at RAND,” October 3, 1963. (A copy of this was kindly provided to us by Joan Goldhamer.)

<sup>33</sup>Interview by Joan Goldhamer and Jim Digby with Bruno Augenstein, May 22, 1986, p. 31. (From Joan Goldhamer’s personal papers.)

<sup>34</sup>Frank R. Collbohm, excerpts from statement before Military Operations Subcommittee, House Committee on Government Operations, August 6, 1962.

<sup>35</sup>Authors’ personal communication with Charles Lindblom (2012).

<sup>36</sup>Grass Roots Critique Folder, Robert Specht papers, RAND Corporation Archives, Santa Monica, CA, p. 1.

<sup>37</sup>Memorandum sent by Hans Speier in 1962 re RAND reorganization, Memo 103152, Folder 51, Box 9, Hans Speier papers, University Libraries, University at Albany, State University of New York.

<sup>38</sup>Memorandum sent by James Schlesinger, “Difficulties in and a mechanism for partial RAND structuring,” January 30, 1967. (A copy was provided to us by James R. Schlesinger.)

<sup>39</sup>Unpublished note by John Williams, “RAND’s self-criticism,” September 19, 1963, RAND Corporation Archives, Santa Monica, CA, p. 11. (A copy was kindly provided to us by Henry Rowen.)

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